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Delivery-date: Fri, 13 Feb 1998 06:36:20 +0000
Date: Thu, 12 Feb 1998 23:35:42 -0700 (MST)
From: ALWYN VANDERMERWE <avanderm@du.edu>
Subject: Re: Ridderbos and Redhead. REPORT 2AR
X-Sender: avanderm@odin.cair.du.edu
To: "m. l. redhead" <mlr1000@cam.ac.uk>
MIME-version: 1.0
X-UIDL: 15309ad20eed302472a972cca4d6e1c9

Dear Authors: Please respond to the report 2AR below. Many thanks.
Cordially, AVDM.

REPORT 2AR

Dear Prof. van der Merwe,

I do not want to stand in the way of publishing this paper especially if there have been other positive referee reports and it is the wish of the journal to do so. In general I think that the topic is interesting and perhaps publishing it will further work on the problems the authors address.

Having said this, I must confess that I'm not convinced by the authors' responses to my report. Let me make a few remarks about the authors' reply.

1. It still isn't clear to me that haven't begged several questions in their rejection of Sklar's line on the spin echo results. They say, of the coarse-graining approach, that its advocates will have to appeal to the history of the system---a move that they (the coarse-grainers) do not have to make in the case of other systems. Isn't it the case that the interventionist must also explain this behavior? In the paper they say that "the kind of thermodynamic behaviour we would like to explain using statistical mechanics is the behaviour which leads to the usual situation in which an innocent observer unaware of the history of the system will actually make the *right* prediction, namely that the syustem is going to stay in the equilibrium state for all future times." Is it the authors' claim that statistical mechanics should not be expected to play a role in the explanation of the innocent's surprise? How does the interventionist explain the fact that the innocent makes the *wrong* prediction? Or, again is that not something that can be/should be explainable by the interventionist?

re words

As I've said, I may very well be missing something here that is important, but it does seem to me as if restricting SM explanations to "true equilibration" begs the question against Sklar who seem to think that the observed anti-thermodynamic behavior is something SM can and should play a role in explaining.

2. Looking back at the paper I see that the authors don't assert explicitly that it is a virtue that their model has no stronger properties than mixing, though it stills seems that the context of the remark is naturally read in that way. The rejection of ergodic approaches as candidates for explanation (because of KAM type results), however, seems to me to be premature. These results may very well play a crucial explanatory role even if the system may be slightly nonergodic.

Question A

3. Finally, I agree that there is an important difference between restricting ones explananda to systems which are small subsystems of larger

systems/environments and the limitations due to features about observational accuracy. On the other hand, I think I may be excused from the criticism of discussing them in the same context given this sentence of the authors' paper: "In the interventionist account, the emphasis is shifted from a limited measurement resolution towards measurements which are restricted to limited, interacting systems."

} omit on p. 17

My main worry here was and is that there is a fairly standard line against interventionist approaches which the authors seem to ignore; namely, that one can always "expand the 'system'" to include part of the larger environment. Then the interventionist line with respect to the original "system" won't seem to apply (or so the argument goes). In effect this is why I asked what justifies the delimitation of the system as the 'system' to be investigated.

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or the
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 X-Authentication-Warning: ruunat.fys.ruu.nl: ridder owned process doing -bs
 Date: Mon, 9 Feb 1998 12:54:48 +0100 (MET)
 From: Katinka Ridderbos <T.M.Ridderbos@fys.ruu.nl>
 Reply-To: Katinka Ridderbos <T.M.Ridderbos@fys.ruu.nl>
 To: mlr1000@cus.cam.ac.uk
 Subject: reply to Van der Merwe
 MIME-Version: 1.0
 X-UIDL: b4d62flf8c0a9720fa63695bd2ffde2c

Dear Michael,

here is a draft of a reply to our referee. I will make some small changes to the paper, in response to the points the referee raised - as soon as I have a new version ready I will send it to London.

best wishes,
 katinka

Dear Professor Van der Merwe,

thank you for sending us the second referee's report concerning our paper "The Spin-Echo Experiments and the Second Law of Thermodynamics". In response to this report we would like to raise the following points. First of all, the referee asks for our motivation for developing our model of the spin-echo experiments. In the literature on the foundations of statistical mechanics, the spin-echo experiments are often quoted as an example of apparent anti-thermodynamic behaviour. These experiments thus play a crucial role in the debate about the appropriate statistical mechanical analogues of equilibrium and entropy. We thought it appropriate to give a complete mathematical account of a simple model of the spin-echo experiments, so as to bring out the conceptual issues in the clearest possible way. As far as we are aware, this model has not been treated elsewhere in the physics literature in such explicit detail. We believe that statistical mechanics is concerned with explaining the approach to true equilibrium, not quasi-equilibrium, as the appropriate analogue for thermodynamic behaviour. Otherwise the spin-echo experiments could be regarded as exhibiting anti-thermodynamic behaviour, which we do not believe to be the case. Our discussion of the "innocent observer" is meant to bring out the problems the advocates of the coarse-graining approach face. It is a short-coming of the coarse graining approach that in this approach the distinction between quasi-equilibrium and true equilibrium cannot be made. In order to avoid being forced to say that the spin-echo systems exhibit anti-thermodynamic behaviour, they will have to appeal to the history of the system, a move they do not make in the case of other systems.

don't of course
 Second, we ~~never~~ claim ~~that~~ it is a virtue of our model that it is a mixing system and has no stronger ergodic properties. On the contrary, our remark about the infinite times the system needs in order to reach the equilibrium state is meant to point at the problematic aspects of approaches based on mixing properties, since we are convinced that statistical mechanics should reproduce the finite relaxation times we find in real thermodynamic systems. The interventionist approach we defend ^{Katinka} in this paper makes no reference to ergodic theorems, and may be expected to produce more realistic relaxation times to true equilibrium even for mixing systems. In general we reject ergodic approaches, since they do not appear to be relevant for realistic systems (~~cp KAM-theorems, etc.~~)

Third, the distinction between a "small" system and a "large" environment seems to us a perfectly intelligible and objective one, and is quite unlike the coarse graining appeal to limitations of observation. Restricting one's ambitions to explaining the behaviour of small, limited systems in terms of the properties of these systems and their environments is not the same as explaining the behaviour of isolated systems in terms of our limited abilities to measure the properties of these isolated systems.

Our reference to those authors who claim an increase for the entropy of the whole universe is intended merely to say how they get their results, not at all to say we agree with them.

Finally, we are grateful for the reference to Prig^oine's paper, which we were not aware of, and which we will include in the paper.

Regards,

Prof. M.L.G. Redhead and T.M. Ridderbos